STANFORD UNIVERSITY
STANFORD, CALIFORNIA

DEPARTMENT OF GENETICS
School of Medicine

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Dr. Bruno Rossi
Physics Department
Massachusetts Institute of Technology
Cambridge 39, Mass.

Dear Dr. Rossi:

I have, believe me, been working very hard on the proceedings of our recent committee meeting at Stanford for consideration of planetary biology. (May I as jargon call this Westex, and the group in your locality, of course, Eastex). As I wrote you earlier, most of our time was devoted to questions of contamination, owing to the imminence of CETEX and of Pioneer IV. We hope to meet again on March 21, at JPL in Pasadena to take advantage of the opportunity of seeing this development laboratory in operation. We expect now to stress the more constructive issues, especially having had some indoctrination and time for meditation.

I should like to stress that the group agreed on a very firm stand on contamination, that we would consider it " a scientific catastrophe and an act of unconscionable irresponsibility" to permit the deposition of a single viable microorganism on the surface of Mars or Venus, pending further information on the habitability of these planets by terrestrial microorganisms. We have not received adequate information (and perhaps it does not exist) on the effects of impact with the surface or of atmospheric friction as would allay the fears of contamination by an uncontrolled mission. However, the group felt that a program of microbiological control could and should be initiated that might demonstrate the feasibility of sterilizing an experimental mission to determine the physical conditions that would control possible habitability. This would involve the use of modern, effective methods of fumigation and empirical verification of their total reliability when applied to sample missiles. It is only commonsense that the first approaches to these planets should be designed to obtain as much information as possible without impact, but the same precautions must be taken to guard against the consequences of accidental landing. Pre-sterilization might be coupled with a structural design that would assure complete incineration by atmospheric friction for these first approaches. I trust that these representations will be in mind not only for CETEX but also for the US NASA plans for Venus shots this June as have been announced by Repr. Brooks.

We suggested that the microbiological research installations at Fort Detrick (cf. Dr. Riley Housewright) and perhaps the Q'master corps labs. at Natick, might be in a favorable position to assist on the control program.

The Westex group was somewhat less emphatic as concerns the moon as a target--less, I feel, perhaps as a measure of scientific caution as of the impression that it might have to be bargained away in order tooensure the integrity of other planetary objectives. I do not entirely share this hesitation, as I feel that the primacy of scientific studies of "life on other worlds" will not be contested by anyone in a responsible position.

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Heartily
Joshua Lederberg

Our present conception of lunar conditions does not permit of the possibility of growth and spread of terrestrial organisms, and the moon is therefore substantially less sensitive as a target than are Mars and Venus. There was some spread of opinion as to the level of deposited biological material (viable or not) that would constitute ultimately detectable, hence signfficant residues of contamination, ranging from 10^5 to 10^{10} microorganisms per missile. Certainly, by present methods a live (though dormant) bacterium or spore would be the most readily detected. Our considered recommendation was therefore (1) that moonshots also be subjected to effective sterilization procedures, and (2) that the level of contamination <u>before</u> sterilization be minimized by clean technique, to not more than 10^8 per missile. From the technical outlines furnished us by Hibbs and Davies, we felt these were quite reasonable limitations and could be met by reasonable diligence without interfering in any way with the engineering program.

The group as a whole was NOT willing to <u>dismiss</u> the possibility of interplanetary transport of spores, having taken into considered account the telling arguments of radiation inactivation and the difficulties of escape. Such transport is considered most <u>unlikely</u> on present knowledge.

Present conceptions of the lunar surface may have to be applied less dogmatically until more information is available from closer approaches. This is illustrated by the fact that the persistence of a local atmosphere (e.g. Alter and Koryzev's observations) can be controversial. The generalizations that have been formulated about temperature ranges, exposure to solar radiation and so forth may well apply to average conditions, and may not preclude local exceptions. If there can be any question of persistent moisture from internal sources, and this would be utterly preposterous except for Koryzev's claims, the moon might surprisingly prove to be a sensitive target, along with the other planets. A similar agnosticism should be applied to predictions of universal conditions on the surfaces of other planets, e.g., the supposed high temperatures prevailing on Venus.

Together with precautionary measures for sterilization, we recommended a detailed molecular inventory, so that later planetary investigators might more easily identify fragments of organic material, alloys, etc. as having originated from a previous missile. This inventory should include all intended components of the missile in terms of atomic and molecular composition and their amounts, and also as accurate estimates as can be made of adventitious materials: dirt, lubricants, fumigants, propellants and particles that may be taken up in flight.

New research is needed to furnish information in the following areas, apart from the obvious necessities of astronomical data:

1) The effects of impact of a missile: particularly dispersion and heating of various components.

2) Flux and penetration of solar UV, X-ray and corpuscular radiation in trans

3) Incineration of missiles in planetary atmospheres (presumably not complete in view of survival of meteoritic earthfalls.)

4) Methods of fumigation of missile payloads and of microbiological control.

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Comments on Cetex-1 report, as published in Science, Oct. 17, 1958

1) The committee felt it would be difficult to place sufficient stress on the importance for theoretical biology of unimpeachable evidence on the status of life on other planets. We now have an increasingly plausible picture of the steps whereby life evolved on earth, so that we have strong expectations for parallel developments elsewhere, wherever the availability of carbon compounds (which must be universal), water or other solvents, and temperatures in a suitable range are compatible with the evolution of chemical complexity in organic (carboniferous) compounds. The unspoiled state of the surfaces of the other planets may be the only means available to the human species ever, and certainly for many years to come, whereby these speculations can be tested by explicit observation.

Laymen and other scientists may be expected to be equally strongly motivated by a fundamental curiosity as concerns the uniqueness of life in the universe to recognize planetary biology as one of eth most fundamental issues in space exploration that will persist when most of the momentary pressures have been forgotten in the perspective of history.

If any errors of judgment are to be made, clearly they must be conservative ones. Would this generation of scientists ever be forgiven by its successors if it permitted the execution of a cosmic blunder that could ~~xxxxx~~ be remotely anticipated? By their very nature, experimental missteps in biology may do irreversible harm; in the physical sciences they may lead at most to exasperati delay and waste.

On the whole, we believe it necessary and possible to formulate a program of space research that conserves objectives in biological science without impeding sober objectives in the physical sciences. Indeed, the two programs are not fundamentally separable.

2) The ~~XXXXXXXXXX~~ Cetex report is a clarifying document that does much to place the start of planetary biology and chemistry in reasonable perspective. We would, however, take exception to some particular points that warrant furthe discussion:

A) 'any contamination of the (moon) dust by space operations will be localized.'...owing to the low density of the atmosphere.

This premise is fundamental to a number of assurances concerning the safety of lunar probes, but can it be supported? No particle will reach the moon's surface with less than escape velocity. Any fragment which recoils having dissipated half or less of its kinetic energy will have sufficient velocity to orbit the moon. Residual energies of less than half will allow for parabolic trajectories to ranges approaching the whole perimeter. The absence of an atmosphere allows for the prompt dispersal of parts of the missile, to <u>any</u> point on the moon's surface. This supposition is concordant with the widely accepted interpretation of the lunar rays, especially Tycho, precisely as the result of fallout from meteoritic infalls. These rays may extend for thousands of kilometers! (Cf. Baldwin, The face of the moon, 1949).

(A more cogent expectation is that any uncontrolled impact may result in the dissipation of most of the kinetic energy as heat. If this can be substantiated for lunar impacts, there would be no danger of biological contamination. However, it appears to be uncertain whether we could rely on impact-heat sterilization of the entire payload; indeed those fragments that were most widely dispersed might be expected to be heated the least, since they woul have dissipated less of their infall energy on the impact. This question plainl has not been exhausted.)

Other means and assurances of localization of missile components must be

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B) Solar radiation would decompose biospores just as it decomposes cosmic dust....

This may be granted for exposed particles lying on a smooth, unprotected surface. The point of exception is obvious: the moon is not such a surface.

It is of course a serious criticism of panspermia, how can a biospore transit the solar radiation field to reach another planet without being destroyed. To sustain the hypothesis we might have to plead that the spore is embedded in some other protecting material, e.g., a particle of clay, or else that some hitherto unknown optical property of the spore in high vacuum might furnish some protection. The former plea makes it more diffiuclt to accept Arrhenius' proposal of radiation pressure as the impetus to interplanetary transit. All this admitted, we do not feel that we have the intimate knowledge of conditions on the lunar surface and in interplanetary space to cast a decisive a priori judgment against the hypothesis.

In conclusion, we feel that general stress on minimizing contamination of any kind and excluding microorganisms as far as technically feasible are plausible parts of any cautious program of investigation. Rather than leave the moon for the uncontrolled deposit of uncontrolled contamination, it should be the testing ground for the same cautions as apply to the more sensitive planets.

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The following scientists participated in the first meeting at Stanford on planetary biology. FEB 21 1959

Institution	Name	Position
University of California (Berkeley)	Roger Stanier	Professor of Bacteriology
	Gunther Stent	Assoc. Professor of Bacteriology and Virology
	Melvin Calvin	Professot of Chemistry
	Dan Mazia	Professor of Zoology
	Harold F. Weaver	Professor of Astronomy
(Davis)	Alan G. Marr	Assoc. Professor of Microbiology
Stanford University	Konrad B. Krauskopf	Professor of Geochemistry
	Joshua Lederberg	Professor of Genetics; Professor of Biology
	C.B. Van Niel	Herzstein Professor of Biology
University of Oregon	Aaron Novick	Director and Professor of Biophysics
California Institute of Technology	Norman Horowitz	Professor of Biology
Stanford Research Institute	Fred Kamphoefner	Director, Control Systems Laboratory
JPL-NASA	A. Hibbs R. Davies	